

savage, cannibal though he be." We cannot inure ourselves to salt at too early an age; we ought indeed to pickle our babies: "To rub new-born infants with salt" is a practice "in every respect cleanly" and "strictly conducive to health."

Mr. Boddy has evidently spent much pains on his history: but, as he confesses, in trying to begin at the beginning he has laboured under many difficulties. He has traced the history of salt from the time of Moses and Job by the aid of such written records as he has been able to meet with, but on the question of its history before their time he is obliged to fall back on his inner consciousness.

"The origin of salt is one of those enigmas of nature which as yet has completely frustrated the most accomplished and scientific geologists, and no suggestion has yet been made which will satisfactorily and conclusively account for its formation; for whatever hypothesis has been stated there is sure to be an objection so difficult to overcome that the author has been fain to admit that it is thoroughly impracticable, and therefore inadmissible."

Even our author is fain to express himself guardedly on this point—

"If we take salt as a whole, leaving out of the question altogether the different conditions in which it is found, and with no reference at all to its existing either in the earth, above the earth, in lakes, or in the sea, but looking at it simply as it is, a mass of rock, or a volume of water holding it in solution, it inclines one to the belief that it possesses a dual inchoation, though the original source of both may have been connate; but owing to extraneous causes which were brought to bear, one branch became crystallised rock-salt, while the other, through immaturity, remains in a state of solution."

"Why the sea is salt" has given rise to many pretty fables: Mr. Boddy invents still another fable; but it is not at all pretty: it is that "sea-water is the result of some subterranean communication with reservoirs of salt through the media of volcanic foci" (p. 53). This perhaps hardly does justice to Mr. Boddy's powers of narrative: the picture of the saltless world proves that he can do better; and yet even this is surpassed by that of the insect world of Cheshire on a rainy day (p. 60). But it is scarcely fair in the interests of the book itself to quote all its best things, even if our space and the reader's patience were longer.

Mr. Boddy is apprehensive of the reviewers: "An unknown author is placed at a great disadvantage and at the mercy of those who may laud a book to the skies if they please, satirically criticise another, and pass over a third with a sarcastic smile or a significant shrug of the shoulders. I am afraid that my little volume will unfortunately be found among the latter, but I candidly acknowledge that I hope it will be regarded as belonging to the first, or at least the second."

Our theory of the origin of this book differs somewhat from that of its author, as given above; Mr. Boddy's father (to whom the book is dedicated) was, we are informed, a ship's surgeon; and it occurs to us that this book is the result of the molecular motion of a brain which can trace its ancestry to a prolonged regimen of salt junk and pickled pork. It is the most striking instance of heredity we have yet met with, and despite our fear that Mr. Boddy may describe our notion as "a brazen assertion and a subtle paralogism," we commend it to the notice of Mr. Francis Galton.

OUR BOOK SHELF

Text-Book of Practical Organic Chemistry for Elementary Students. By H. Chapman Jones. 100 pp. (London: Joseph Hughes, 1881.)

MOST teachers of organic chemistry have felt that if their students could be made to work through a fairly simple series of typical experiments the work of learning would be rendered easier, and the knowledge gained would be made more definite and more real. Just such a series of experiments is described in this little work by Mr. Chapman Jones. The experiments are well chosen and clearly described; no costly apparatus is required, yet the student who works carefully through the book will certainly have laid a solid basis of knowledge of organic chemistry on which he may build a satisfactory structure.

An outline of methods whereby organic acids may be detected is given towards the end of the book, but the main part is devoted to experiments illustrative of fractional distillation and precipitation, formations and general properties of leading hydrocarbons, alcohols, and acids, etherification, &c.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Trevandrum Observatory

AS I was reading in a recent issue of your valuable journal (vol. xxiii. p. 482) a letter on the magnetic storm of August, 1880, showing the universality and simultaneity of the disturbance by comparing the observations at Greenwich, Toronto, Zi-ka-Wei, and Melbourne, I felt curious to know whether any such disturbance was noted here in the Government Observatory, and if so, whether the time corresponded with that given in your paper. On my application the gentleman in charge of the Observatory put into my hands, the inclosed abstract for the whole month of August, which I herewith forward to you. It contains, as you will see, not only the magnetic observations with the unifilar, bifilar and balance, but also the meteorological data for the necessary correction, &c. The reference throughout the paper is to the local time, which may be easily reduced to the Greenwich time, as the longitude is given. The observations, I may add, are quite reliable, though made by native agency, and I hope may prove useful on this occasion. But the paper inclosed I fear is too long to find room in your crowded columns, and what I beg you to do is to place it at the disposal of any of your scientific contributors or friends who take an interest in the question of terrestrial magnetism, and may be therefore expected to make use of the material here furnished.

A word more before I close. Your readers might know this observatory, said to be situated near the magnetic equator, was once in a very flourishing condition under the direction of Mr. John Allan Broun. On his retirement to Europe the establishment was reduced and a limited series of observations introduced, which he continued to direct till his recent demise. Since then the observations recorded are lying unused for the want of a scientific chief. If any scientific gentleman or society should generously offer some help in the way of directing the labours of this institution, I venture to think that the Government would gladly avail itself of such help, and the cause of science could then be materially promoted.

P. SOONDREM PILLAY
H. H. the Maharajah's College, Trevandrum, Trevancore,
South India, May 6

Symbolical Logic

FRESH criticism of my logical writings in a work just published ("Symbolic Logic," by John Venn, M.A., Fellow and Lecturer in the Moral Sciences, Gonville and Caius College, Cambridge) must be my excuse for troubling the editor and readers of NATURE with a third letter on the above subject.

On page 94 of his work Mr. Venn strikes the key-note, as it seems to me, of its whole purport and spirit. "Those who propose a new notation," he says, "commonly, and not unnaturally, assume that it is to supersede all others. But those who approach it as strangers know that the odds are decidedly that it will only prove one more of those many attempts which perplex and annoy the lecturer, historian, and critic. Hence we may fairly use the argument, dear to those in authority, that if we loosen the sanctions of orthodoxy, heresies will multiply. Only those whose professional employment compels them to study a number of different works have any idea of the bewildering variety of notation which is already before the world. . . . No doubt it would be rank intolerance to forbid such new attempts, but an attitude of slight social repression towards them may serve to check too luxuriant a growth of new proposals."

The italics are mine. Alas, how little Mr. Venn appreciates the irrepressible restlessness of that most ungovernable organ, the human brain, if he really thinks that the "attitude of slight social repression" which he recommends would have the desired effect! Amateur logicians, as well as professionals, will start theories and invent notations of their own in happy unconsciousness that they are causing any annoyance to "lecturers, historians, and critics," whom indeed they not improbably picture to themselves (when the all-absorbing nature of their occupation allows them to think of them at all), as ardent devotees of science like themselves, who will be delighted with the new instrument of research which they hope to place in their hands. And more provoking still, scientific societies and editors (including a goodly number of the said lecturers, historians, and critics) will print in their *Proceedings* and magazines new proposals which they think likely to prove interesting or valuable, without being influenced by any motive whatever beyond a pure and simple desire to further the progress of science.

Mr. Venn professes great admiration for the late Prof. Boole's genius, and I heartily agree with him, though we admire on somewhat different grounds. I ground my admiration on the fact that Boole worked wonders with an unnecessarily complicated and otherwise defective symbolical method of his own invention. Mr. Venn apparently grounds his admiration on the singular supposition that Boole's method is really very simple and very effective, but that its author did not understand very clearly the real principles of its construction, and did not by any means apply it with as much ease and dexterity as he might have done. I am quite sure that this is not the impression which Mr. Venn intended to create in the minds of his readers, but I am no less sure that this is the impression which a perusal of his book will create in their minds—at least in the minds of such as have not read Boole himself. One remark of Mr. Venn's surprises me. He says (p. 385) that Boole "justly regarded his problems in Probability as the crowning triumph of his system." Surely I am not mistaken in my impression that I have somewhere seen Mr. Venn quoted as holding an opinion very much at variance with this statement—in fact attacking as erroneous the very principle on which Boole's "General Method in Probabilities" is based. May I ask Mr. Venn this plain question, Does he or does he not agree with Boole's solution of the question which he proposed on pp. 321 and 336 of his "Laws of Thought" as a decisive "test of the sufficiency of received methods," and (by implication) of the efficacy of his own General Method?

The main points on which Mr. Venn and I differ are the following:—

1. Mr. Venn maintains that the sign + in such expressions as $x + y + z$ should in logic, as in ordinary algebra, be always understood in an *exclusive* sense, so that unless we know x , y , z to be mutually exclusive, the above expression should be written in a different and, as he admits, a much less simple form. I hold, on the contrary, in common with Prof. Jevons and several others, that since, on the non-exclusive plan, the simple form $x + y + z$ may, without the slightest risk of ambiguity, be substituted at any stage of an investigation for any of its exclusive equivalents (such as $x + x'y + x'y'z$), or be replaced, if necessary, by any exclusive equivalent, the non exclusive interpretation of the symbol + gives us far more mastery over our symbolical expressions, and should therefore be preferred to the needlessly restrictive and hampering exclusive interpretation which Boole attaches to this symbol. How very serious the disadvantages of this interpretation really are is unwittingly illustrated by Mr. Venn himself on p. 262, where he finds himself obliged to admit that certain important simplifications which he discusses are "purely a matter of tact and skill, for which no

strict rules can be given." If he had read my third paper in the *Proceedings* of the London Mathematical Society a little more attentively he would have found in my directions for reducing any complex disjunctive expression to its "primitive form" that these simplifications are not at all a pure matter of tact and skill, but may be obtained by a simple, never-failing, and purely mechanical process, which, however, a little tact and skill may do much to abbreviate. On the exclusive interpretation of the symbol + this process would be simply unmeaning. The problem which Mr. Venn discusses (expressed in my notation) is this:

Reduce the expression $(f:g)(gl:f')(l:f')$ to its simplest form.

By inspection [since any implication $a:\beta$ is equivalent to $a\beta':0$, and any compound implication of the form $(a:x)(\beta:x)(\gamma:x)$ to a single implication $a + \beta + \gamma:x$] this, on the non-exclusive plan, is seen to be equivalent to $fg' + glf' + lf:0$.

Reducing the disjunctive antecedent of this implication to its *primitive form* (a purely mechanical process, as already remarked), we get $fg' + gl:0$, or its equivalent, the compound implication $(f:g)(g:l')$, a result which Mr. Venn obtains apparently by a vague tentative process "for which no strict rules can be given."

2. Mr. Venn and I also hold different opinions as to whether or not symbolical logic should have signs to express relations corresponding to those of subtraction and division in mathematics. His opinion is that such signs *should* be introduced, and at once. My opinion (an opinion which I believe I share with most logicians) is that we had better not encumber ourselves with those symbols till they can be proved to subserve some useful purpose. The important question is—not, as Mr. Venn appears to think, whether such symbols can be intelligibly interpreted, but whether they will in any way help us in discovering new truths; in other words, whether they can be turned to any practical use in the solution of logical problems. If Mr. Venn can adduce a single intelligible logical problem which can be solved more simply or easily by the help of these signs than without them, I shall declare myself at once a convert to his views. So far I have come across no such problem, and must therefore for the present remain in the ranks of his opponents. As an illustration of what Mr. Venn calls the inverse method (*i.e.* division) in logic, he gives (p. 266) the equation—

$$(x + \bar{x}y)w = x + \bar{x}yz,$$

in which w (which denotes the books in a certain library) is to be expressed in terms of x , y , z (which respectively denote *philosophical* books, *divinity* books, and *protestant* books). His result is—

$$w = x + \bar{x}yz + \frac{0}{0}\bar{x}\bar{y},$$

which he translates into ordinary language thus:—

"The library must have certainly contained all philosophy and protestant divinity, and may possibly have contained any kind of works which are neither philosophy nor divinity; this latter constituent being left entirely indefinite."

Mr. Venn's data in the non-exclusive notation would be—

$$(x + y)w = x + yz,$$

and my result (much more simply and easily obtained) is—

$$x + yz:w:x + y' + z.$$

According to my definitions of my letter-symbols, we speak throughout of some *one* originally unclassified book, so that w , x , y , z will respectively denote the statements: It is in the *library*; It is a *philosophical* work; It treats of *divinity*; It is a *protestant* work. My result may therefore be read:—

Any work on philosophy or protestant divinity will be found in the library; and every work in the library is either philosophical, secular, or protestant. (By a *secular* work I mean simply a work that does *not* treat of divinity.)

The antecedent of w in my result is equivalent to the first "constituent" in Mr. Venn's result; but the consequent seems to me to give us much more intelligible information about the library than Mr. Venn's latter constituent ("May possibly have contained," &c.), which he truly describes as "entirely indefinite."

3. Another opinion of Mr. Venn's, unless I misunderstand him, is that all logical equations should, as a preliminary to their solution, be expressed in the form $a = 0$, which, of course, is equivalent to my $a:0$. My opinion is that this, in logic as in mathematics, is sometimes convenient and sometimes not, and that we should not in logic, any more than in mathematics, tie our hands by this or any other unnecessary restriction.

The other points in which I differ from Mr. Venn have been mentioned in my former letters (see *NATURE*, vol. xxiii, p. 578, and vol. xxiv, p. 5), and need not be here repeated. Mr. Venn however labours under a serious misapprehension if he thinks that I attach any importance to the distinguishing features of my method as mere barren conceptions. Their real importance lies in the use which I have made of them, and this use cannot be fairly appreciated without examination of my published solutions in the *Proceedings* of the London Mathematical Society, in the *Educational Times*, and in the *Philosophical Magazine*. I must protest against that spirit of criticism which would offer two or three chipped bricks as a fair specimen of a house, and would depreciate the labours and damp the zeal of all scientific workers by unduly emphasising the undeniable fact that all logical and mathematical methods are, after all, mere combinations, developments, or extensions of a few simple truths which are the common property of all mankind. Even Boole's "actual originality," Mr. Venn tells us, though I think he means *priority*, "was by no means so complete as is commonly supposed and asserted" (see Mr. Venn's Introduction, p. 28). According to this method of criticism we might ascribe the invention of the steam-engine to the person (unfortunately unknown) who first discovered the important principle of a revolving wheel, and turned it to practical account by making a wheel-barrow.

In conclusion I must thank Mr. Venn for the compliment which he kindly pays me on p. 372 of his work, but I think it would read better without the parenthesis, "as he assures us is the case." The "assures" is a little too strong for the simple statements which I made, and which it never occurred to me that any one would dream of doubting. The compliment would also please me more if it did not so completely ignore the earliest, the most difficult, and the most important of my papers in the *Proceedings* of the Mathematical Society, namely, that which treats of the limits of multiple integrals. This part of my method (which gave rise to all the succeeding developments) resembles nothing, so far as I know, that has preceded it; and if Mr. Venn had found time to read it, the objectionable parenthesis which I have quoted would scarcely have presented itself to his mind as in any way called for.

HUGH MCCOLL

Boulogne, May 25

Resonance of the Mouth-Cavity

SINCE communicating to Mr. Sedley Taylor my recent observations on the capabilities of the mouth as a resonator, and forwarded to you, with my permission, for publication in *NATURE*, I have made the following experiments with perfect success, and believing that they will be interesting to your acoustical readers I send a list of them to you, and hope you will be able to find a place for its insertion in your next issue:—

Experiment I.—While one of the overtones of a loud prime was resounding in my mouth another person heard it distinctly, upon the ear of the latter being held near the source of resonance.

Experiment II.—While a cart—any other noisy vehicle will do as well—was going along the street, I readily tested the composite nature of the noise by the resonant capabilities of my mouth.

Experiment III.—I turned the water-tap on into a basin (the water was running with a good force), and from the noise made by the falling liquid I was able to get different sounds quite easily.

In both these latter experiments I observed, while opening and closing my mouth, that the pitch rose and fell as when one slides a finger up and down a vibrating fiddle-string.

Experiment IV.—I held down in the treble part of a harmonium—with an 8-feet stop out—several consecutive keys together, and while the notes were sounding which composed the horrible discord, I was able to single out any of them separately by the sympathetic resonance of my mouth.

This experiment can be done equally well at the organ.

Experiment V.—I held down four or five of the low keys of the organ with the 8-feet trumpet drawn, and the beats of the overtones resounded very prominently, so that by taking two contiguous ones at a time the result was like the *voix céleste* stop.

Experiment VI.—I tried several notes in the *c-c* octave of the clarinet organ stop, and heard the regular order of partial tones by resonance, but the even ones were weak and odd ones very strong.

Experiment VII.—I held down *c'* and *g'* on the harmonium,

and heard the first coincident partials beat distinctly—due to equal temperament.

Experiment VIII.—The first five partial tones of an average bass voice were studied. The notes chosen for observation were *dp, f, g, dp, bp*. To each note the vowels *A* as in *hay*, *A* as in *ah*, *E* as in *me*, *I* as in *high*, *O* as in *oh*, *U* as in *you*, were sung.

It was found that not only did the different vowels give different qualities, but that the same vowel had a different quality for almost every one of the five notes sung. The *E* as in *me* and *U* as in *you* had generally weak low overtones. The *I* as in *high*, on the contrary, gave them out well.

It will perhaps be as well to say, for the benefit of those who may not have tried to get a sympathetic resonance of the mouth, that success is likely to be sooner obtained by first practising the mouth in going from the *ou* to the *ah* shape, and from the *ou* to the *e* shape.

It is also recommended that the ears be stopped by the fingers when doing these experiments, in order to lessen the possibility of mistaking the direct sound for resonance.

JOHN NAYLOR

5, West Park Terrace, Scarborough, June 1

"How to Prevent Drowning"

I FEAR that if persons who cannot swim place reliance on the advice given by Mr. MacCormac in your impression of June 2 (vol. xxiv, pp. 62, 101) they will hardly succeed in saving their lives should they happen to fall into deep water. It is an error to say that the "human frame, bulk for bulk, is lighter than water," for unless that frame be covered with fat beyond the average, it has a greater specific gravity than water. And after all, a tolerably fat body is lighter, bulk for bulk, than water only by virtue of the air in its lungs, and should that air be expelled by the frantic screams of the immersed person, he will soon find, if unable to swim, that the notion of his frame being specifically lighter than the water is a myth. The dead body even of a tolerably fat person being destitute of air in the lungs, sinks at once to the bottom in salt as in fresh water. The average human being, were he to permit his body to sink as far as it will, would soon find himself at the bottom of the sea or river. Besides, even in the case of a person fat enough to be lighter, bulk for bulk, than water, it is necessary that he should assume a certain position in order that he may succeed in keeping his nose and mouth above the water, and unless he learn how to do this in the water itself I doubt if instruction on dry land would ever enable him to float. We all know the story of the Hibernian who, having narrowly escaped drowning, vowed he would never enter the water again until he had learnt to swim, but we are not told if he ever qualified himself for going into the water again. To try and persuade people that by attending to certain rules they may get into deep water without the risk of drowning is to create a false confidence which will rather increase than diminish the number of deaths by drowning. Imagine a terrified person just plunged for the first time into deep water trying to recall all the directions he has read about shutting his lips, swallowing his breath, permitting his body to sink until it shall displace as much water as equals the body's weight, treading the water, and so on. Why he would require, in the midst of his agony of fear, to possess as many contradictory qualities of mind as Macbeth says no man can have. I venture to assert that no one was ever saved from drowning by following such directions as your correspondents here give. It should be stated in the plainest manner that there is no safety for a person in deep water but in a knowledge of swimming. Swimming should be taught to every boy and girl as a necessary branch of education. It has these advantages over much that is taught in schools, that it is a useful, a delightful, and a healthful accomplishment.

R. E. DUDGEON

52, Montagu Square, W., June 3

Dust-winds at Hankow

DURING the spring of 1878 my attention was directed to the dust-winds which are not of unfrequent occurrence along the valley of the Yang-tse in the warm and dry seasons of the year. These dust-winds, as I observed them at Hankow, had sometimes the appearance of a dense mist; whilst at other times the air seemed to be penetrated by a fine haze; and in all cases a fine and almost impalpable dust was deposited